NEW YORK STATE COLLEGE OF AGRICULTURE
CORNELL UNIVERSITY
ITHACA, NEW YORK

LABORATORY OF BACTERIOLOGY

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Dr. Joshua Lederberg
Department of Genetics
University of Wisconsin
Madison 6, Wisconsin

Dear Josh:

Enclosed are a couple more pedigrees from H168. I was a little suspicious of the small one so did not want to invest the time necessary to carry it any farther. The original cell proved to be a heterozygote so it would have been all right. The series 5 cultures, I think, are the most extensive group yet and I intend to carry them out this far, hoping that the segregation will occur early enough that we can perhaps account for the possible inviable nucleus. This series 5 case, cultures 239 and 240 with the inviable 5-120, would be interesting if we did not already have several observations indicating that the sib cell to a segregant is a viable heterozygote.

I am also sending out the following cultures from the 5 series:

203, 204, 195, 196, 25, 53, 54

just on the chance that they may be valuable, although I think probably all of them are heterozygotes. I am checking them here further but I think it is best to get them to you as quickly as possible. I am dumping some of the original broth culture into some melted agar to attempt to cut down the amount of growth before the cultures get to you.

I am not planning on giving anything at Cincinnati. If you wish to use any of the stuff we have so far you are welcome to do so. Actually, there is not a great deal to say beyond the indication that the mechanism is more complex than one might have hoped for. I hope to have several more pedigrees as extensive as this series 5 before the Cincinnati meetings.

Have you played around with cold shocks or any other mechanism for increasing the frequency of the segregation? It would be a big help to me if there were some way of inducing the segregation to occur at a bit higher rate. I have made a few random notes on the back of the series 5 sheets which may or may not indicate that I can spot a cell which will not grow. Actually, I think if I were more careful I could raise my predicting ability to something like 80 per cent accuracy. The main reason I am interested in this is that I feel the failure of cells to grow can not be laid entirely to the rigors of the technique and that there is an actual physiological difference which arises before I separate the sibs. As to culture 2-200 I am positive that at

the time I plated it truly segregating heterozygous cells were present in the culture. I will replate in an effort to confirm this. Actually I have never classified a culture as heterozygous on the basis of its being a mixture of + and - cells. I raised the question earlier because it was a possibility.

What is your guess as to the nature of the aberrant region? Just ~~that~~ a deficiency or a deficiency plus some ~~to the~~ after change or is the situation so confused that one can not make a good guess?

Does Mtl- revert readily? If so, I will switch to xylose.

Very truly yours,

M. R. Zelle

MRZ:jc

Enclosure

Josh,
H 72 was derived from a cross of
BM Het x TLB, Loc-Mal-Gal-Xyl-Arab- V_1^r
but was + for Mal, Gal, Arab. Is this correct?
Also - H 168 is homo or hemi-zygous for Mal-, arab-, ???
Thanks for the xylose, ours came in the same day I got your letter.

H168 genotype? = $\frac{B\ M\ V_K^+\ Het\ + + + + + + + + + + +}{+ + + +\ T\ L\ B,\ Loc,\ Mal\ Xyl\ Gal\ Arab\ Mtl\ V_1^+}$